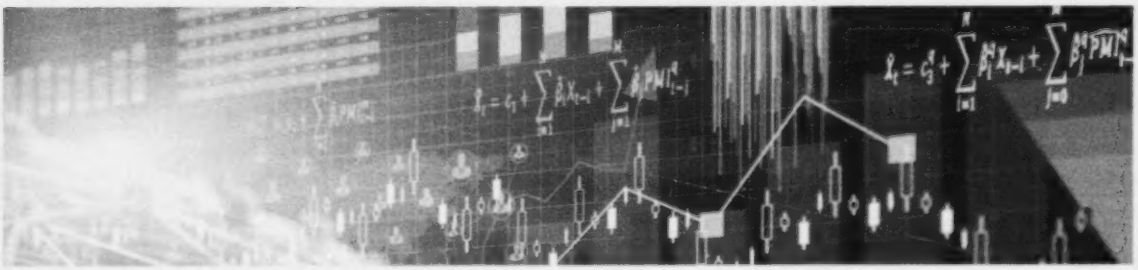
BANK OF CANADA
BANQUE DU CANADA

Staff Working Paper/Document de travail du personnel 2016-40

# Downward Nominal Wage Rigidity in Canada: Evidence from Micro-Level Data



by Dany Brouillette, Olena Kostyshyna and Natalia Kyui

Bank of Canada staff working papers provide a forum for staff to publish work-in-progress research independently from the Bank's Governing Council. This research may support or challenge prevailing policy orthodoxy. Therefore, the views expressed in this paper are solely those of the authors and may differ from official Bank of Canada views. No responsibility for them should be attributed to the Bank.

www.bank-banque-canada.ca

Bank of Canada Staff Working Paper 2016-40

August 2016

# Downward Nominal Wage Rigidity in Canada: Evidence from Micro-Level Data

by

**Dany Brouillette, Olena Kostyshyna and Natalia Kyui**

Canadian Economic Analysis Department
Bank of Canada
Ottawa, Ontario, Canada K1A 0G9
dbrouillette@bankofcanada.ca
okostyshyna@bankofcanada.ca
nkyui@bankofcanada.ca

ISSN 1701-9397

© 2016 Bank of Canada

## Acknowledgements

We gratefully acknowledge the helpful comments of Robert Amano, Allan Crawford, Yuriy Gorodnichenko, Thomas Lemieux, Andy Levin, Rhys Mendes and Pierre St-Amant. We also thank Statistics Canada and the Federal Research Data Center for providing access to the SLID data, as well as Jean-Michel Billette for advice and help in working with the data.

The views expressed in this paper are those of the authors and not necessarily those of the Bank of Canada.

## Abstract

We assess the importance of downward nominal wage rigidity (DNWR) in Canada using both firm- and worker-level microdata. In particular, we analyze employer-level administrative data from the Major Wage Settlements (MWS) and household-based survey data from the Survey of Labour Income Dynamics (SLID). MWS data cover large unionized firms in Canada, while SLID is a rich rotating panel representative of the employed population in Canada. Combining both sources of information allows for a more extensive analysis of DNWR in the Canadian labour market. The results suggest that, on average, the effects of DNWR added about 0.2 to 0.4 percentage points to wage growth between 1994 and 2011; as well, the estimated effects increased in the years following the Great Recession in 2008–09. That includes a higher proportion of workers affected by DNWR (which rose from 16 to 32 per cent) and a larger impact on average wage growth. DNWR's effects on average wage growth were also much stronger during periods of lower CPI inflation in Canada and are positively related to provincial unemployment rates. Finally, we provide an extensive analysis of the heterogeneity in the effects of DNWR. For example, its impact is more pronounced among smaller firms, lower occupational levels, immigrants and older workers. Overall, population ageing and an increasing proportion of immigrants may continue to increase the effects of DNWR in Canada, while the continuing shift toward service industries, declining unionization rates and the increasing educational attainment of the Canadian population may reduce them.



*Bank topic(s): Labour markets; Econometric and statistical methods*
*JEL code(s): J3, E24, J30*



## Résumé

Nous évaluons l'importance de la rigidité à la baisse des salaires nominaux au Canada au moyen de microdonnées sur les entreprises et la main-d'œuvre. Plus précisément, nous analysons les données administratives des Grands règlements salariaux, obtenues auprès d'employeurs, ainsi que les données de l'Enquête sur la dynamique du travail et du revenu, recueillies auprès des ménages. Tandis que les premières portent sur les grandes entreprises syndiquées au Canada, les deuxièmes proviennent de riches panels rotatifs et représentatifs de la population occupée au Canada. La combinaison des deux sources de données permet de réaliser une analyse plus complète de la rigidité à la baisse des salaires nominaux sur le marché canadien du travail. Les résultats donnent à penser qu'en moyenne, les effets de cette rigidité ont fait monter la croissance des salaires d'environ 0,2 à 0,4 point de pourcentage entre 1994 et 2011. De plus, les effets estimés ont pris de l'ampleur après la Grande Récession de 2008-2009. Par exemple, la rigidité à la baisse

des salaires nominaux touche une plus grande proportion de travailleurs (cette proportion est passée de 16 % à 32 %) et a une incidence plus marquée sur la croissance moyenne des salaires. Les effets de la rigidité sur la croissance moyenne des salaires ont également été beaucoup plus importants pendant les périodes où l'inflation mesurée par l'IPC était plus faible au Canada; il existe par ailleurs une relation positive entre ces effets et les taux de chômage provinciaux. Enfin, notre étude fournit une analyse détaillée de l'hétérogénéité des effets de la rigidité à la baisse des salaires nominaux. Celle-ci est notamment plus prononcée chez les petites entreprises, les personnes n'occupant pas de postes de cadres, les immigrants et les travailleurs âgés. Somme toute, le vieillissement de la population et l'augmentation de la proportion d'immigrants au sein de la population active pourraient continuer d'accentuer les effets de la rigidité à la baisse des salaires nominaux au Canada, alors que la tertiarisation de l'économie, la baisse des taux de syndicalisation et la hausse du niveau de scolarité de la population canadienne pourraient les réduire.

*Sujet(s) : Marchés du travail; Méthodes économétriques et statistiques*
*Code(s) JEL : J3, E24, J30*

**Non-technical summary**

Downward nominal wage rigidity (DNWR) is present in the labour market when workers and employers are reluctant to implement cuts in nominal wages. This can arise for many reasons, for example, because nominal wage cuts could negatively affect workers' morale or be perceived as unfair. The presence of DNWR is generally regarded as an impediment to wage adjustment in the economy. For example, there is usually downward pressure on wages and employment when the economy runs slowly (e.g., during a recession). In the presence of DNWR, however, nominal wages would not be cut but would be kept at high levels, so employment might drop even further than it would without DNWR. An increase in the proportion of workers whose wages are frozen may point to the presence of DNWR, but more comprehensive analysis is needed, since some wage freezes would have been implemented anyway.

Our objective is to assess the presence and extent of DNWR in the Canadian labour market. We combine two data sources—Major Wage Settlements and the Survey of Labour Income Dynamics—to conduct a more complete statistical analysis of DNWR. The results suggest that (i) between 1994 and 2011, the effects of DNWR added about 0.2 to 0.4 percentage points, on average, to wage growth, and (ii) the effects of DNWR increased in the years following the 2008–09 recession, when the percentage of workers affected by DNWR was higher and the impact on average wage growth was larger. Moreover, DNWR's effect on average wage growth was much stronger during periods of low inflation and high unemployment. We also provide evidence that DNWR's effects differ by individual and firm characteristics. Overall, population ageing and an increasing proportion of immigrants may continue to increase the effects of DNWR in Canada, while the continuing shift toward service industries, the declining unionization rate and the greater educational attainment of the Canadian population may reduce them.

Finally, what does the presence of DNWR imply for the unemployment rate in Canada? The evidence in the economic literature is mixed; some studies conclude that DNWR increases the long-term unemployment rate, while others reach the opposite conclusion. Therefore, the presence of DNWR is not sufficient by itself to conclude that it has a real impact on unemployment; more research is needed to shed light on this issue.

# 1 Introduction

Evidence in the economic literature suggests that firms and workers may be reluctant to implement cuts in nominal wages for many reasons, including money illusion, a history of unionization, maintaining workers' moral or creating a perception of fairness. From a macroeconomic perspective, the issue of downward nominal wage rigidity (DNWR) is important, as this type of rigidity can hamper the adjustment in the labour market following, for instance, a negative demand shock. In response to such a shock, all else being equal, nominal wages should decline to accommodate the lower quantity of labour demanded at a new equilibrium. The distribution of nominal wage changes would thus shift to the left, and a higher proportion of workers would get wage freezes and cuts. But in the presence of DNWR, a larger share of wage changes would remain at a zero bound – assuming that DNWR is binding at zero – rather than resulting in cuts. Wages would not completely adjust, or would adjust more slowly, to the shock, which might result in a higher unemployment rate. While this negative trade-off between inflation and unemployment has been well known since the seminal paper of Tobin (1972), whether the impact of DNWR is permanent is still debated (see, for example, Fortin (2013) for a discussion). The first step, however, is to assess the extent of DNWR, which is the primary objective of this paper.

Previous studies for Canada suggest that wage cuts were rare, but that wage freezes were frequently observed (see, for example, Simpson, Cameron and Hum 1998). Using Major Wage Settlements (MWS) data, Crawford and Wright (2001) estimate that DNWR added from 0.1 to 0.4 percentage points (pp) to average wage growth during the 1990s. Simpson, Cameron and Hum (1998) find slightly higher estimates of DNWR (0.7 pp) for the 1993–95 period. Amirault, Fenton and Laflèche (2013) report evidence of aversion to base nominal wage cuts using data from the Bank of Canada's Wage Setting Survey. Crawford and Harrison (1998) also report that DNWR is higher in base pay than in total compensation and higher in unionized firms. Finally, Crawford (2001) shows that the incidence of wage freezes is higher during periods of low inflation.

There is also evidence of DNWR in other countries. For example, Card and Hyslop (1997) and Kahn (1997) find some evidence of DNWR in the US Panel Survey of Income Dynamics (PSID). Altonji and Devereux (2000) and Lebow, Saks and Wilson (1999) also point to strong evidence of DNWR in the United States. More recently, using PSID data, Dickens et al. (2007) estimate that DNWR prevented 28 per cent of the wage cuts. Using the establishment-based compensation survey from the Bureau of Labor Statistics, Fallick, Lettau and Washer (2016) find evidence of significant nominal wage rigidity in the United States.[1] In their study, about 16 per cent of wage changes are zero, and the size of this spike is negatively related to the inflation rate. The proportion of wage changes constrained by DNWR is about 50 per cent, and the proportion of jobs subject to DNWR is about 36 per cent over the estimated period. Fehr and Goette (2005) find evidence of DNWR in the Swiss Labour Force Survey and Social Insurance Files and that it is quantitatively important and robust across different estimation models: the share of workers affected by DNWR is estimated to be between 48 and 54 per cent. Also, DNWR is more prominent in a low-inflation environment.[2] Using Dutch administrative data, Deelen and Verbeek (2015) estimate that the fraction of Dutch workers affected by DNWR ranges from 12 to 52 per cent. They also find that estimates of DNWR are higher when inflation expectations are lower, namely for 2009–12 relative to 2007–08.[3]

We assess the importance of DNWR in Canada using micro data on firms (for a given position) and workers (for persons keeping the same job) from the Major Wage Settlements (MWS) database and the Survey of Labour and Income Dynamics (SLID), respectively. The starting point for our analysis is to extend the paper of Crawford and Wright (2001) by using more recent data from MWS. Then we also explore

1 This survey is used to compute the Employment Cost Index. The unit of observation in this survey is jobs, not workers.

2 DNWR prevented wage cuts for one-third of job-stayers, and the average prevented wage decrease was 2.7 per cent in 1991 (inflation of 5 per cent). Then, in 1997 (inflation of 0 per cent, after four years of low inflation), cuts were prevented for 62 per cent of job-stayers, and the average prevented wage decrease was 6.5 per cent.

3 Other examples of studies for the United States include Akerlof, Dickens and Perry (1996); Gottschalk (2005); Daly, Hobijn and Lucking (2012); Daly and Hobijn (2014); and Robertson (2015); and Babecky et al. (2009) for European countries.

SLID data, which have never been used for this purpose. While the MWS and SLID databases have both advantages and disadvantages, the SLID data allow us to strengthen our conclusions and explore dimensions that cannot be explored with the MWS data. At the same time, a similar US data source – the PSID – was used in several studies mentioned above, some of which, interestingly, have reached different conclusions about the presence of rigidity by focusing on different aspects of the same data.[4]

The main contribution of this paper to the literature is that it provides a range for the impact of DNWR for Canada by combining job-level (MWS) or worker-level (SLID) micro data. Our assessment is that both datasets contain useful and complementary information about the extent of DNWR in the labour market. For instance, while the MWS data provide reliable information about base pay changes, they only cover unionized organizations employing at least 500 workers in Canada. On the other hand, SLID data are representative of the entire working-age population in Canada, but since the database consists of self-reported data, it may suffer from reporting errors. SLID richness, however, allows us to better analyze the heterogeneous effects of DNWR across Canadian workers, something that is not possible to do with the MWS data. Also, to evaluate the effects of DNWR, we introduce a new measure of DNWR based on a notional wage distribution. In particular, we compute the amount by which average wage growth is higher because workers received wage freezes instead of wage cuts estimated by the notional distribution. We also provide an estimate of the percentage of workers affected by DNWR, which has not been reported before for Canada.

We find evidence of DNWR in both the SLID and MWS data. The impact of DNWR on average wage growth varies depending on the data source and wage measure. On average between 1994 and 2011, the results suggest that the effects of DNWR added 0.2 to 0.4 pp to wage growth. Based on both data sources, we find that the estimated

[4] Card and Hyslop (1997) and Kahn (1997) find rigidity in wages by focusing on the spike at zero in the wage-change distribution, while McLaughlin (1994) concludes that wages are flexible by emphasizing the symmetry of the wage-change distribution and its large left tail.

impact of DNWR on average wage growth increased in the years following the 2008–09 recession. Likewise, the proportion of workers affected by DNWR was larger after 2009. Moreover, the impact of DNWR likely increased post-2011, as suggested by the MWS data.

We also find evidence that higher effects of DNWR are associated with lower inflation and higher unemployment, based on provincial inflation and unemployment rates. In particular, the effect of DNWR on average wage growth was much stronger during periods of low CPI inflation in Canada: the effect was more than five times larger when inflation was below 1.5 per cent compared with periods when inflation was above 2.5 per cent. Estimation results also suggest that higher unemployment rates are associated with larger effects of DNWR, which points to the costs of DNWR in terms of higher unemployment. We also observe an increase in the effects of DNWR following the 1991 recession (based on MWS) but that increase was much larger than the increase following the 2008–09 recession, even though the inflation rate was higher in the early 1990s, despite similar levels of wage freezes. The results thus suggest that the increase in wage freezes after 2008–09 was not only driven by lower inflation but, to a large extent, by the weakness in the economy, given that inflation levels were higher in the early 1990s.

The remainder of the paper is organized as follows. Section 2 describes our data sources and presents descriptive statistics and key stylized facts. Section 3 discusses our empirical methodology. Section 4 describes estimation results, and Section 5 concludes.

# 2 Data

## 2.1 Major Wage Settlements (MWS)

Several previous studies on DNWR in Canada use Major Wage Settlements (MWS) data, collected by the Labour Program at Employment and Social Development Canada. The period covered in our paper is from January 1978 to May 2015. MWS data are a reliable data source for negotiated wage changes in Canada and have been used in a number of studies to assess the extent of DNWR. The main advantage of MWS data is that they are void of any measurement or reporting errors. MWS data are also available over a longer period than SLID, which may be useful for comparing the importance of DNWR before and after the introduction of Canada's inflation-targeting regime in 1991, as well as around several recessions, including the Great Recession. It also permits comparison with previous studies.

MWS data, however, only cover unionized organizations in Canada—whether private or public—with at least 500 employees.[5] This is an important caveat, as less than 0.2 per cent of all firms in the private sector had that many employees in 2013, although they employed about 37 per cent of private sector employees.[6] Moreover, only about 25 per cent of workers are unionized in the private sector in Canada, while this proportion is 75 per cent in the public sector.[7] Another caveat is that MWS reports a negotiated change in the base wage rate for only the lowest-paid job category; for a fraction (if not a majority) of workers the actual change in hourly wage could differ significantly from the change in the base wage rate. It may thus be difficult to generalize the results to the whole economy. Seeking an alternative data source with broader coverage of the economy—and other measures of wages—is thus advisable to complement our analysis.

Two measures of wage changes are available in MWS. The first is the wage change

[5] For simplicity, unless specified, firms refer to both private firms and public organizations.

[6] Statistics Canada CANSIM Tables 527-0002 and 527-0006

[7] Also note that the share of agreements signed in public sector organizations increased from 55 per cent in 1977 to about 70 per cent in 2015 in MWS.

for the first year of the contract. The second is the average wage change over the lifetime of the contract. Our analysis mainly focuses on the first measure, as wage freezes are more likely to happen in a given year than on average over several years. Effects of DNWR on wage growth are qualitatively similar when using average wage changes, although the magnitude is much smaller.

## 2.2 Survey of Labour and Income Dynamics (SLID)

The Survey of Labour and Income Dynamics (SLID) is a subsample of the Labour Force Survey (LFS). SLID is a longitudinal household survey that provides data on income over time for individuals in Canada, which makes it well suited for the purpose of the current analysis.[8] The first panel was introduced in 1993 and consists of about 20,000 households, which stayed in the sample for six years.[9] Starting in 1996, a new panel was introduced every three years, so two panels always overlapped. For the period from 1996–2011, each new panel consists of approximately 17,000 households.

While SLID was never used to study DNWR, it has been widely used to study various labour market outcomes in Canada. For instance, Schirle (2012) studies wage losses of displaced older men by using average wage rates during the year, computed as individual wages and salary in each year divided by the number of hours worked at all jobs that year. Gray and Qiu (2010) examine the flexibility of average real wages by industry in response to labour demand shocks using one of the wage measures we use (implicit hourly wage for a paid job). Hum and Simpson (2004) estimate the impact of immigration on labour market performance as measured by composite hourly wage rates.[10]

We use data for workers aged 16–69 years.[11] We exclude individuals reporting at least

[8] Respondents stay in the LFS sample for six months; wage data are not collected every month.

[9] For comparison, the PSID sample is smaller; it covers 5,000 households.

[10] Other studies using SLID data on wages include Yoshida and Smith (2005, 2008) and Gauthier-Loiselle (2013). Yoshida and Smith (2005, 2008) study earnings differentials between visible minority immigrants and native-born workers. Gauthier-Loiselle (2013) estimates the incidence of extended maternity leave benefits on relative wages.

[11] SLID does not collect labour market information for individuals younger than 16 and older

one non-paid-worker job in a reference year, such as self-employment. We also restrict our sample to individuals employed at their main job in December of each reference year who did not experience any unemployment or out-of-labour-force spells during the year. The wage measure we use is derived from self-reported wages and total hours worked during a year. This measure includes tips, commissions and bonuses, but does not include paid overtime. The SLID wage data represent total compensation, in contrast to the MWS data, which measure a change in the base pay. From a firm perspective, however, flexibility in total compensation could be more important than flexibility in the base pay, given that total labour costs encompass bonuses, commissions, and other pay. We use two wage measures in our computations: end-of-year wages (wages received in December of a year) and composite wages (average wages over a year).

We estimate DNWR for job-stayers—workers not changing jobs—so that the observed wage changes are not contaminated by wage changes due to job changes. We define job-stayers as individuals who started their main job on or before January 1 of a year prior to the reference year, so their current job tenure is at least 24 months. We calculate the wage change between two years based on two measures of hourly wages: "end-of-year wages" (implicit hourly wage as of December) and "composite hourly wages" (average wage over a year).

One potential problem with SLID is that the data are self-reported, which means that the hourly wage measures may suffer from reporting errors.[12] We have applied further restrictions to our sample in order to limit the potential impact of self-reporting errors. First, we compare total compensation calculated on self-reported data and the total compensation reported in the linked tax information from the Canada Revenue Agency. The latter information includes total compensation as reported in T4 slips, as well as other payments to employees by employers not reported in T4s, such as research grants, tips, gratuities or directors' fees. These two measures differ slightly by

than 70.

[12] We use hourly wage as opposed to income over a period, as the latter may change due to a change in hours worked.

definition, because compensation reported for tax purposes includes payments that are not requested SLID, such as paid overtime, pay for a parental leave, the value of taxable benefits, sold or bought additional vacation days, etc. However, large discrepancies between these two measures may indicate a problem with the self-reported data or the fact that additional payments constitute a larger fraction of income than wages. We thus exclude individuals whose self-reported total wage earnings from SLID differ from wage earnings reported in their tax declaration by more than 200 per cent (that constitutes 3.0 per cent of the sample), as well as individuals with imputed wages in any of two periods used for the wage-change calculation (11 per cent), and individuals with a large variation in their tax-file information from one year to another (that constitutes 3.8 per cent). We also exclude individuals having missing values in any of the key variables used in the empirical analysis.

Additionally, we exclude large outliers in wage changes, as they most likely reflect reporting errors or unidentified job changes. This practice is consistent with Altonji and Devereux (2000); Fehr and Goette (2005); Dickens et al. (2007); Deelen and Verbeek (2015); and Fallick, Lettau and Wascher (2016). We first remove observations with wage cuts of more than 50 per cent and wage increases of more than 100 per cent for any of these two measures (representing 1.4 per cent of the sample). Then, in the empirical analysis, following Dickens et al. (2007) and Deelen and Verbeek (2015), we further restrict the sample of wage changes of each measure to the interval of [-35 per cent, 60 per cent]; by doing so we lose 2.7 per cent for the end-of-year wage measure and 1.5 per cent for the composite wage measure. Estimates using this range will give us results comparable with those of studies for other countries. For robustness analysis, we also use a more restrictive set by allowing wage changes to be in the interval of [-30 per cent, 30 per cent]; in this case we lose an additional 5.7 per cent and 4.1 per cent of the end-of-year and composite wage changes, respectively. The latter sample is also more consistent with the observed mean wage changes from other data sources, LFS and the Survey of Payrolls, Employment and Hours (SEPH), as we discuss below. The estimation results reported in the next section are robust to different cut-off levels.

Since the wage-change analysis is performed only for job-stayers, it is important to highlight how this subsample of the employed population differs from the overall population of workers. Job-stayers are more likely than the average worker to be males, older than 25 years old, more educated, employed in the public sector, have a full-time job, be unionized, have supervisor responsibilities, and employed in large firms. They are, however, less likely to be enrolled in an educational establishment during a reference year, to be immigrants or part of a visible minority. Further details are in **Table B1**.

## 2.3 Wage dynamics

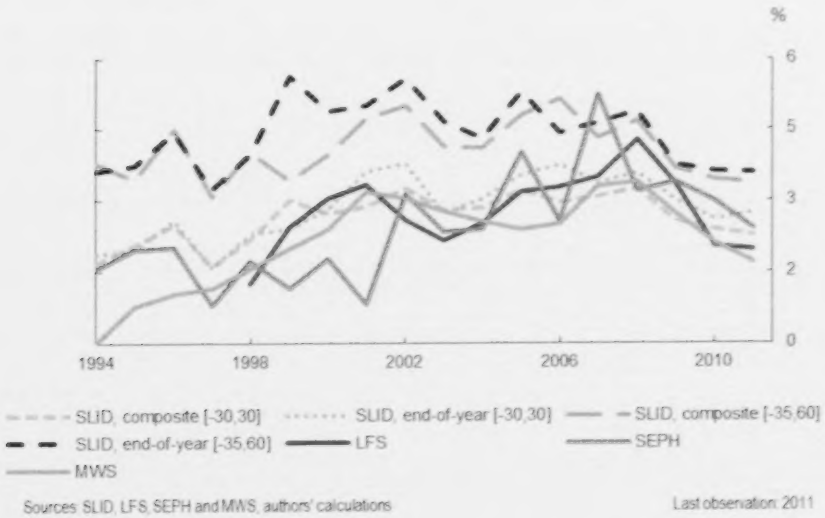
**Figure 1** shows the average wage growth using MWS and SLID data and wage growth from LFS and SEPH—two of the most frequently used data sources for labour market information, including wages. SLID shows average wage growth rates that are very close to those in LFS, SEPH and MWS when we consider restricting the sample to wage growth rates in the range of [-30 per cent, 30 per cent]. Nevertheless, all of these wage measures move broadly together, although they show some volatility. Wage growth was increasing up to 2008 and then declined following the 2008–09 recession. For example, average wage growth fell from 3.4 per cent from 2001–08 to 2.8 per cent after the 2008–09 recession based on the SLID composite wage change in the range of [-30,30]. LFS wage growth was 3.1 per cent and 2.5 per cent, and MWS growth was 2.9 per cent and 2.2 per cent during these periods. The dynamics of inflation were similar: annual CPI inflation decreased from 2.3 per cent to 1.7 per cent during the same periods. The variance of wage growth has also declined over time, which motivates using the time-varying variance in our model.[13]

We find that wage freezes are quite frequent in SLID data, as evidenced by a large spike at 0 in the histogram of wage growth rates based on end-of-year wages (**Figure 2**). In our sample, wage freezes account for a large share (30 per cent), while the wage cuts are less prominent (20 per cent of wage changes).[14] The spike at 0 can be interpreted as an indication of the presence of DNWR in the data. Card and Hyslop

[13] The variance fell after 2004 compared with the 1994–2003 period.

[14] The histogram with composite wage changes is similar.

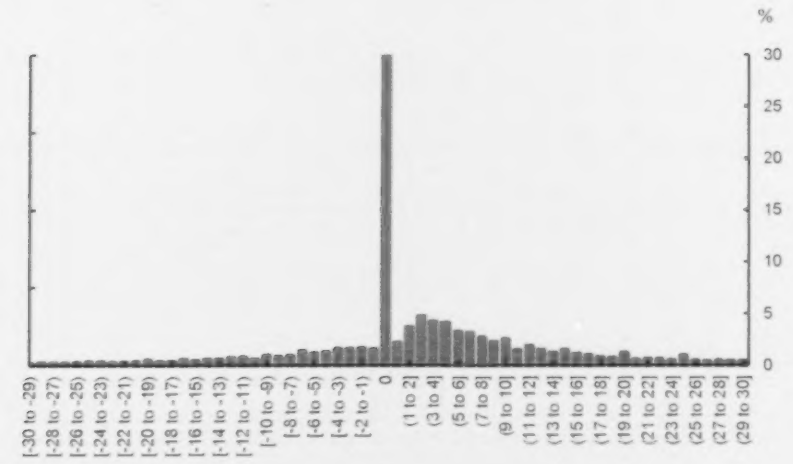
**Figure 1: Wage growth rates, year-over-year (%)**




(1997) and Kahn (1997) also find evidence of DNWR in the US PSID data by focusing on the substantial spike of wage changes at 0, rather than on the cumulative density below zero. The share of wage freezes in our sample is larger than in these studies, while the share of wage cuts is similar. For example, Card and Hyslop (1997) report that 7 per cent to 10 per cent of respondents report constant wages, while 15 per cent to 20 per cent of respondents experienced wage cuts. Kahn (1997) reports 7 per cent of respondents with constant wages and 18 per cent with wage cuts. However, other researchers interpret the same PSID data differently. McLaughlin (1994) concludes that wages are not rigid, emphasizing the symmetry of the distribution of the wage changes and its large left tail. Interestingly, McLaughlin (1994) reports 7 per cent of constant wages and 17 per cent of wage cuts, which are almost the same as in Kahn (1997).

Similar to Simpson, Cameron and Hum (1998), we find that in the MWS data wage cuts are very rare, but wage freezes are frequently observed (**Figure 3**). While MWS shows a spike at 0 in the histogram of wage growth rates, these data also feature larger spikes at positive wage growth rates than those observed in the SLID data. This could

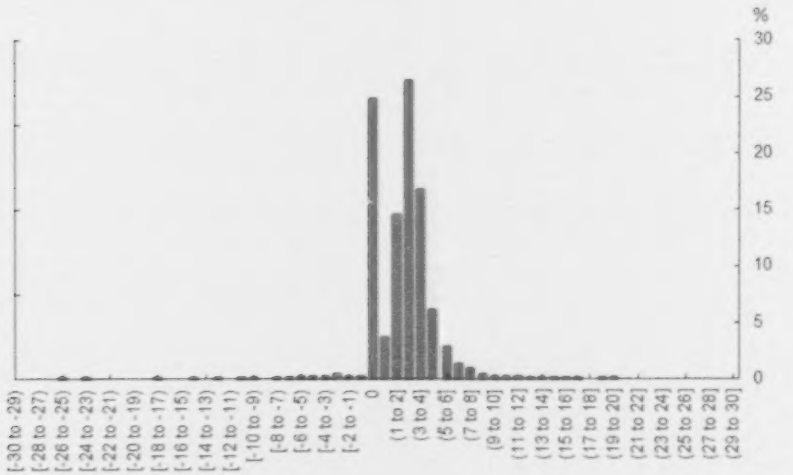
**Figure 2: Histogram of wage growth rates, 1994–2011 (SLID)**




Sources: SLID, authors' calculations

be an artifact of the MWS data populated by the large unionized organizations and dominated by the public sector.

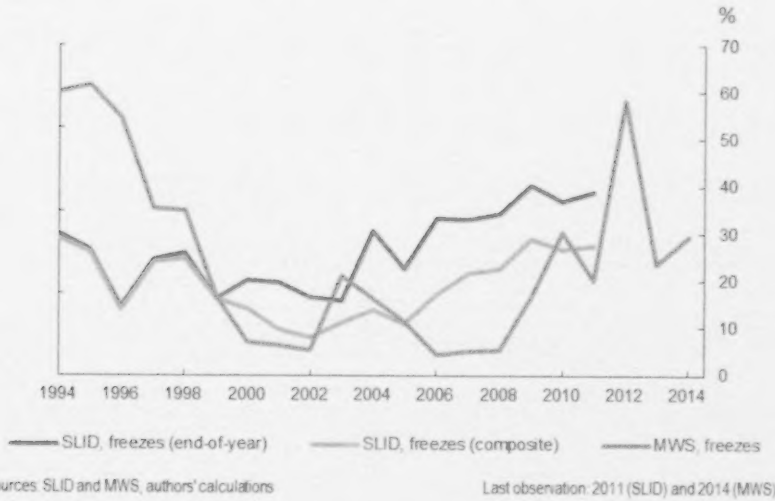
**Figure 3: Histogram of wage growth rates, 1994–2011 (MWS)**




Sources: MWS, authors' calculations

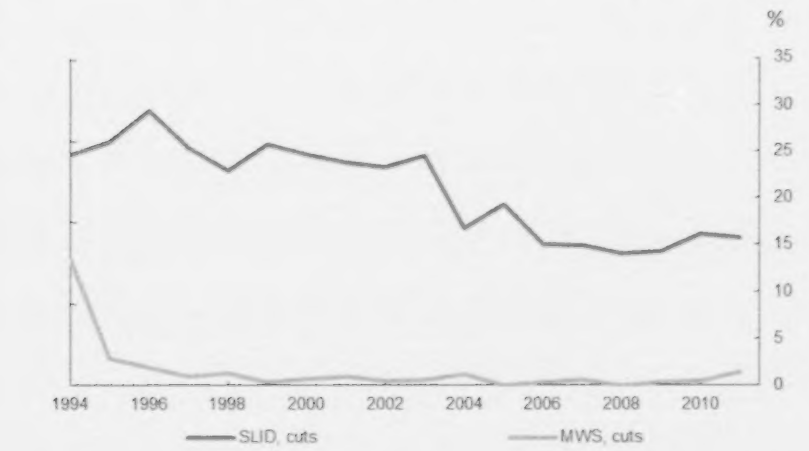
Understanding the dynamics of wage freezes and cuts over time is also important for the study of DNWR. **Figure 4** shows that the share of wage freezes increased gradually in the SLID data starting in 2004, while the share of wage cuts has fallen since 2004 (**Figure 5**). The increasing share of wage freezes combined with the falling share of wage cuts suggests higher DNWR since the mid-2000s than in the late 1990s. In the MWS data, the share of wage freezes increased following the 2008–09 recession, mostly due to more wage freezes in the public sector (**Figure 4**). The share of wage cuts has remained low and stable (**Figure 5**).

**Figure 4: The fraction of wage freezes increased following the recession of 2008–09**




Since 2004, wage freezes have been somewhat less frequent in the MWS data than in SLID, but the number of wage freezes in MWS spikes almost to 60 per cent by 2012 (**Figure 4**). The most prominent episodes with wage freezes in MWS happened in the mid-1990s and early 2010 in the wake of the 1991 and 2008–09 recessions, with almost all of the freezes observed in the public sector. These differences largely stem from different samples in MWS and SLID. In particular, the MWS data include only large unionized firms with more than 500 employees, while the SLID data are representative of the overall labour market in Canada. In the SLID data, we find that wage freezes are much more frequent among small firms than large firms: shares

**Figure 5: We observe fewer wage cuts since 1994 in SLID, while cuts are very rare in MWS**




Sources: SLID and MWS, authors' calculations

Last observation: 2011

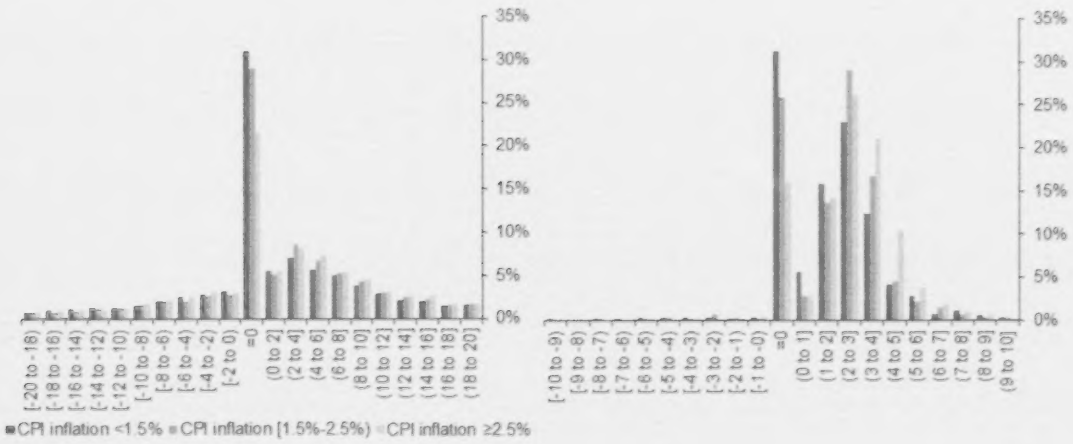
of wage freezes are about 31 per cent in firms with less than 100 employees and 24 per cent in firms with more than 500 employees. Interestingly, Altonji and Devereux (2000) find a higher frequency of wage freezes (40 per cent) in the data from a large financial corporation than in PSID (7 per cent to 10 per cent) and a lower frequency of wage cuts (2.5 per cent or less).

The implications of the differences between MWS and SLID data for our analysis are twofold. First, it is likely that the public and private sectors are affected differently by DNWR. While the MWS data are dominated by the public sector, overall employment (and SLID) is not, and this is partly why Crawford and Wright (2001) focused on the private sector only using MWS. Second, since wage cuts are more frequent in the SLID than in MWS, we need to introduce a modified measure of DNWR compared with the measure used in Crawford and Wright (2001) to account for the much larger left tail in the distribution of wage growth (see Section 3).

Similar to other studies, we find that the incidence of wage freezes is higher when

inflation is lower. **Figure 6** illustrates that the proportion of wage freezes is higher in the provinces with a lower level of regional CPI inflation (based on both SLID and MWS data for the same period, 1994–2011). The share of wage freezes is 31, 28 and 24 per cent when total CPI inflation is below 1.5 per cent, between 1.5 and 2.5 per cent and above 2.5 per cent, respectively (cited numbers correspond to the left panel of **Figure 6**, SLID data; fractions of freezes in the MWS data are at a similar level, though the differences by CPI levels are even more pronounced, as suggested by the right panel). Altonji and Devereux (2000) find that the proportion of wage freezes is inversely related to the inflation rate. Deelen and Verbeek (2015) also find that estimates of DNWR were higher when inflation expectations were lower, namely for 2009–12 relative to 2007–08.

**Figure 6: Wage freezes increase when inflation decreases**



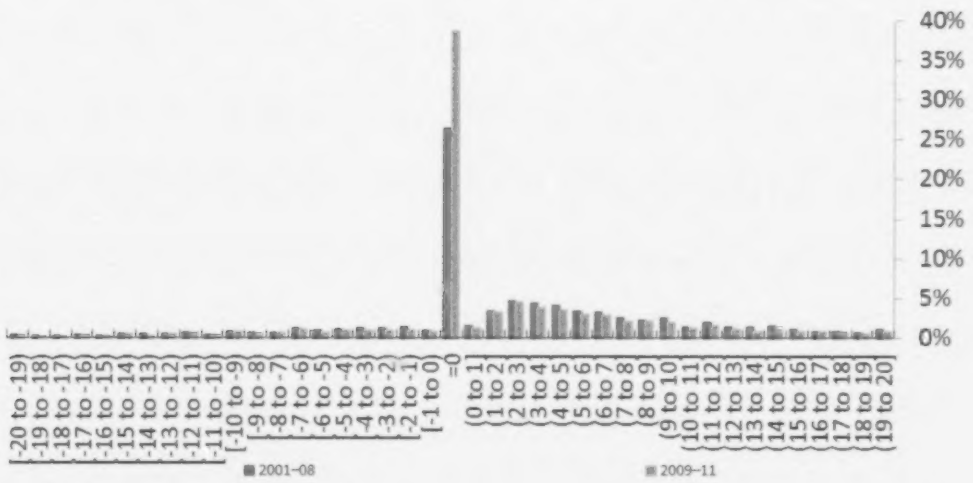

Sources: SLID and MWS, authors' calculations

Another important observation from **Figure 6** is that the distribution of wage changes is wider in the SLID data than in the MWS data. That could be largely explained by the measure of wages: MWS reports changes in the base wage rate of the lowest-paid job category, while SLID reports changes in the total compensation (including wages, bonuses, tips, and commissions), adjusted by the change in hours worked if such a

change occurred as well. It is intuitive that the variation in total compensation per hour should be expected to be larger than the variation in the base wage rate. [15] It is, however, remarkable that the size of the spikes at zero are very comparable across two datasets on average over the analyzed period.

**Figure 7** shows that the distribution of wage changes became more concentrated around 0 in the post-recession period (2009-11) relative to the pre-recession period, while the left and right tails of the distributions did not change much. The higher incidence of wage freezes after the recession is consistent with the increase in the proportion of freezes during this period (**Figure 4**). One minor caveat regarding **Figure 7** is that the difference in the percentage of freezes is biased upward by the choice of reference period: the proportion of freezes only started increasing after 2004, while it was fairly stable between 2001 and 2003. Using the 2004–08 period instead of the 2001–08 period would have led to a smaller difference in the spike at zero in **Figure 7**. The overall conclusion would remain the same, however.

**Figure 7: Wage freezes increased after the recession**




Sources: SLID, authors' calculations

[15] Even more variation is observed in earnings per hour based on tax-file earnings, which include even more components than self-reported earnings in SLID. See Appendix A for details.

Finally, the data suggest that wage freezes are more frequent among the following groups of workers: senior workers (over 55 years), those with lower educational levels, employed in the private sector, working part-time, employed in goods-producing industries, working in smaller firms, and working in non-supervisory roles, as well as among immigrants and visible minorities.

## 2.4 Inflation expectations

Nominal wages are determined by several factors, such as local employment conditions—captured in our model by the unemployment rate—and expectations about changes in future prices. Therefore, inflation expectations are an important piece of information in our assessment of the effects of DNWR.

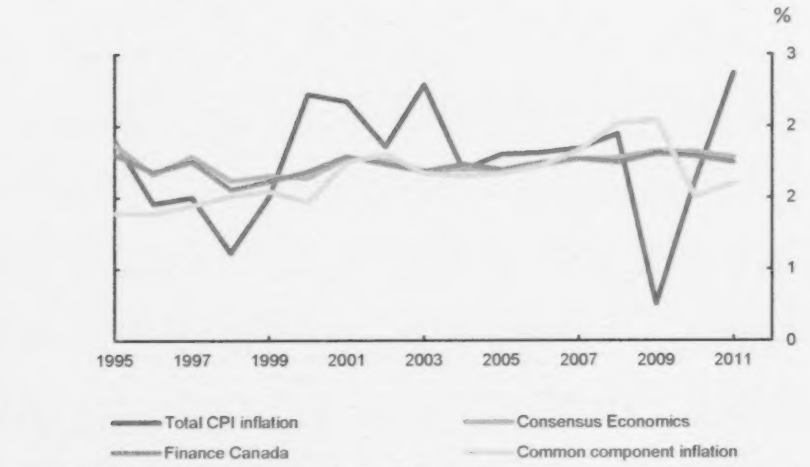
There can be different ways of estimating inflation expectations. First, we can rely on measures from surveys of forecasters, such as Towers Watson; inflation expectations from the survey of business confidence by the Conference Board of Canada; the survey of private sector forecasters by the Department of Finance Canada; and the Consensus Economics survey. These surveys provide inflation expectations over different horizons and at different frequencies. We use three-year-ahead expectations for estimations with the MWS data, since the average duration of contracts is about 30 months. For SLID data, there is no information on the frequency at which wages were revised, so we assume a yearly basis.

We also use a simple backward-looking mechanism for forming inflation expectations, where the inflation expected for a given year is the inflation observed in the previous year, with inflation being measured as the growth rate of the total consumer price index (CPI). Total CPI inflation is, however, subject to short-term fluctuations due to temporary factors and relative price changes, so we also perform estimations using the Bank of Canada's common component of inflation (see Khan, Morel and Sabourin 2013). Finally, market-based inflation expectations can be inferred from asset prices. We have used the measure of average expected inflation computed by the Financial

Markets Department at the Bank of Canada based on its macro-finance model.

While different measures of inflation and inflation expectations move generally in the same direction, their values can be quite different on a period-by-period basis (**Table B2**). **Figure 8**, as expected, shows that common component inflation exhibits the least volatility of these four measures (by design, the common component eliminates some idiosyncratic volatility). We can see that from 1994 to 2000, actual inflation is below survey measures of expectations, while from 2000 to 2008, the values of all four measures are quite close. In the post-recession period, common component inflation indicates higher inflation, while the survey measures of expectations are close to total CPI.

**Figure 8: CPI inflation and selected measures of inflation expectations**




Sources: Statistics Canada, Consensus Economics, Department of Finance Canada and Bank of Canada, authors' calculations

Last observation: 2011

# 3 Empirical methodology

## 3.1 The Tobit model

Following Crawford and Wright (2001), we use a Tobit model with time-dependent variance and a stochastic threshold to derive the notional wage distribution. In addition to DNWR, nominal wage changes could be pushed to zero because of the presence of menu costs. In particular, it may be more costly for firms to offer a small wage increase, between 0 and k, rather than a wage freeze. As a result, when a small increase is expected, firms may freeze wages instead. Such freezes would not be related to DNWR, and the model needs to be adjusted to account for that. Since there is little information available on the magnitude of the menu costs, we follow Crawford and Wright (2001) and use a Tobit with a stochastic threshold to account for menu cost (Nelson 1977). This threshold is thus endogenously determined and estimated along with other parameters. Therefore, our Tobit model is defined as follows:

$$\Delta w_{it} = \begin{cases} \Delta w_{it}^{n} & \text{, if } \beta X_{it} + \epsilon_{it}^{n} > k \\ 0 & \text{, if } \beta X_{it} + \epsilon_{it}^{n} \leq k \end{cases} \tag{1}$$

where $\Delta w_{it}^{n}$ is the observed change in hourly wages. The indexes $i$ and $t$ represent workers and time, respectively. The notional wage distribution, and thus the distribution of wages in the absence of DNWR, is then given by $\Delta w_{it}^{n} = \beta X_{it} + \epsilon_{it}^{n}$. The parameter $k$ represents the stochastic threshold and is defined as follows: $k = k_0 + \mu_{it}$, $\mu_{it} \sim N(0, \sigma^{\mu})$. The random term in the notional wage equation is specified as follows: $\epsilon_{it}^{n} \sim N(0, \sigma_t^{n} = N(0, \exp^{(\alpha_0 + \alpha_1 t)^2})$ and we assume that it is uncorrelated with $\mu_{it}$. Previous studies, for instance, Crawford and Wright (2001) and Crawford (2001), have shown that the variance of the wage distribution has decreased through time, which is also the case with our data.

We use the Tobit model in this study mainly for the following two reasons: i) to reassess the impact of DNWR with a longer MWS dataset using the same methodology

as in earlier studies for Canada; ii) to have a similar empirical strategy for both data sources, MWS and SLID. Certainly, given the richness of the SLID data, there is scope for developing and using more flexible empirical models, which could allow us to get a more comprehensive view of wage dynamics in Canada. We discuss these potential directions below, but leave it for future research to explore other approaches. This paper, using a relatively simple methodology, allows us to have comparable results across two different datasets and relative to the earlier studies, which has a value on its own and constitutes a first step towards a further, more extensive analysis of DNWR in Canada.

## 3.2 Measures of DNWR

We then use the estimated parameters from Equation (1) to assess excess average wage growth due to DNWR, as well as the percentage of workers affected by DNWR. For the MWS data, we again follow Crawford and Wright (2001) and define the effect of DNWR on wage growth as the difference between the estimated mean wage in the presence of rigidity and the estimated mean of the notional wage-change distribution ($\hat{\beta}X_{it}$):

$$RIG = Prob\left(\hat{\beta}X_{it} + \epsilon_{it}^{n}\right) E\left[\hat{\beta}X_{it} + \epsilon_{it}^{n} | \hat{\beta}X_{it} + \epsilon_{it}^{n} > 0\right] - \hat{\beta}X_{it} \tag{2}$$

This measure of the effect of DNWR works well for MWS, since most wage changes are positive (**Figure 5**). In fact Equation (2) holds under the assumption that "all contracts with pressures for nominal-wage cuts receive a wage freeze" (Crawford and Wright 2001, p.4). But with a higher proportion of wage freezes and cuts, as in the SLID, we need to find alternative measures that better handle the left tail of the wage-change distribution. We define the following DNWR measures:

- $RIG_A$ evaluates the extent to which the average growth in wages is higher because people with wage freezes did not receive wage cuts as predicted by the estimated notional distribution ($\widehat{\Delta w_{it}^{n}} = \hat{\beta}X_{it}$).

$$RIG_A = \begin{cases} 0 - \Delta\widehat{w_{it}^n} & \text{, if } \Delta w_{it}^n = 0 \text{ and } \Delta\widehat{w_{it}^n} < 0 \\ 0 & \text{, otherwise} \end{cases} \tag{3}$$

- $PR_A$ estimates the proportion of workers affected by these respective measures of DNWR.

# 4 Estimation results

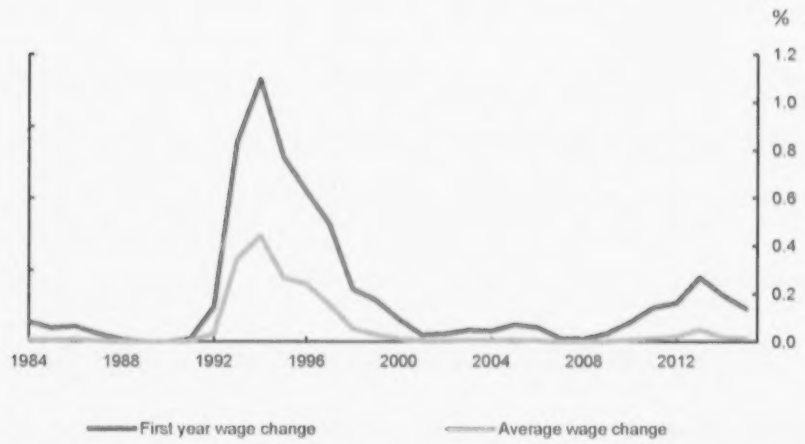
## 4.1 Estimates of DNWR from MWS

Two measures of wage changes are used in the estimation: first-year changes and average change over the duration of the contract. **Table B3** shows a selected set of estimated parameters. The measure of wage rigidity used is RIG, as in Crawford and Wright (2001) and defined in Section 3. We find that our results are robust to the choice of the inflation expectation measures (**Table B4**). Overall, wage growth was about 0.2 pp higher due to the presence of DNWR between 1978 and 2015. The same effect is lower, at less than 0.1 pp, when the average wage change is used. Consistent with Crawford and Wright (2001), using the latter wage-change measure yields smaller estimates of the impact of DNWR than using the former. This arises from having more wage freezes in the first year than over the duration of contract.

When analyzing the results over time, however, we see some variation in the effects of DNWR (**Figure 9** and **Table B4**). We find little evidence of DNWR in the 1980s, an expected result, given the high inflation rates experienced over this period—thus making wage freezes a rarity. The impact of wage rigidity on average wage growth, however, increased in the wake of the 1991 recession—reaching a peak in 1994 of 1.3 pp—but then steadily decreasing towards zero until 2008, as shown in **Figure 9**. The impact of DNWR again increased between 2009 and 2015. The peak, at 0.2 pp in 2013, is, however, much lower than after the 1991 recession. The estimated

impact in 2015 is about 0.1 pp, back to its level of 2000. Overall, the post-recession dynamics are similar following the 1991 and 2008–09 recessions; the main difference is the magnitude of the effects of DNWR on wage growth despite similar levels of wage freezes (**Figure 4**). The results also suggest that the increase in wage freezes after 2008–09 was not only driven by lower inflation but, to a large extent, by the weakness in the economy — inflation levels were higher in the early 1990s.

**Figure 9: DNWR estimates using MWS data, percentage points, by year**




Sources: MWS, authors' calculations

Last observation: 2015

The dynamics are, however, mostly driven by negotiations in the public sector. For instance, the peak impact after the 1991 recession is 1.8 pp (1994) for public organizations and 0.8 pp (1993) for private firms. Likewise, the post-Great Recession peak for the public sector was 0.5 pp in 2013, while the effect of DNWR was around 0.1 pp for the private sector between 2011 and 2015. This suggests that wages in the public sector are much more rigid than in the private sector. This is an interesting result as, by construction of the MWS dataset, this difference cannot be attributable to unionization. One possible explanation is that unions could have more bargaining power as their membership increases, making it more difficult for employers to cut wages — the number of unionized workers per organization is almost 50 per cent

higher in the public sector than in the private sector. And, indeed, when calculating the effects of DNWR on wage growth, the estimates get slightly larger for the largest firms. More work would be required to shed light on this issue.

## 4.2 Estimates of DNWR from SLID

**Table B5** reports the estimated coefficients for the two wage-change measures described above, using SLID data. The table presents results using provincial CPI inflation as the measure of inflation expectations. We also tested other measures of inflation expectations, and the estimation results are not sensitive.[16] The estimation results suggest that inflation expectations have a strong positive effect on wage changes, with the coefficient ranging from 0.4 to 0.8, depending on the wage measure used and the restrictions on the sample. The provincial unemployment rate has a negative effect on wage changes. Other coefficients have the expected sign and magnitude; we thus do not discuss them and move directly to the estimated effects of DNWR.

**Table 1** reports the estimated effects of DNWR on average wage growth ($RIG_A$ measure). The estimated effects of DNWR do not vary much depending on the inflation expectations measure used, but do vary for different measures of wage changes. Overall, the impact of DNWR on wage growth ranges between 0.1 and 0.4 percentage points (pp), depending on the wage measure used. The results by period are also broadly consistent with what is suggested by the dynamics of wage changes shown in the previous section. We find that the effects of DNWR varied between periods and increased in the wake of the recent recession. The impact on average wage growth due to DNWR in 2009–11 more than doubled relative to 2001–08 for all wage measures. However, comparing the 1994–2000 and the 2009–11 periods, we find that the effects of DNWR are higher for the end-of-year wage, but lower for the composite wage.

As mentioned earlier, we exclude observations with a wage change higher than 60

[16] Measures used with the SLID data are provincial CPI, CPI common component, Towers Watson and Conference Board.

**Table 1: Estimated effects of DNWR on wage growth by period (percentage points)**

| | Wage chage [-35%;-60%] | | Wage chage [-30%;-30%] | |
|---|---|---|---|---|
| | EoY | Comp. | EoY | Comp. |
| *Effects of DNWR on average wage growth* | | | | |
| All years | 0.44 | 0.10 | 0.30 | 0.06 |
| Years: 1994–2000 | 0.36 | 0.18 | 0.27 | 0.13 |
| Years: 2001–2008 | 0.29 | 0.02 | 0.20 | 0.01 |
| Years: 2009–2011 | 0.95 | 0.10 | 0.60 | 0.05 |
| *Effects of DNWR on average wage growth, % of average wage growth (all years)* | | | | |
| Average wage growth | 4.28 | 4.05 | 2.58 | 2.83 |
| Effects of DNWR | 10.30 | 2.40 | 11.60 | 2.10 |
| *Effects of DNWR on average wage growth, by subcategories* | | | | |
| Provincial Inflation: < 1.5% | 0.79 | 0.21 | 0.55 | 0.14 |
| Provincial Inflation: 1.5–2.0% | 0.37 | 0.07 | 0.25 | 0.04 |
| Provincial Inflation: 2.0–2.5% | 0.31 | 0.06 | 0.20 | 0.03 |
| Provincial Inflation: >2.5% | 0.16 | 0.02 | 0.10 | 0.01 |
| *Effects of DNWR—subsample equivalent to MWS* | | | | |
| Unionized firms, 500+, non-supervisory: 1994–2000 | 0.31 | 0.16 | 0.23 | 0.11 |
| Unionized firms, 500+, non-supervisory: 2001–2008 | 0.22 | 0.01 | 0.14 | 0.00 |
| Unionized firms, 500+, non-supervisory: 2009–2011 | 0.85 | 0.08 | 0.52 | 0.04 |

EoY: End-of-year wage change; Comp.: composite wage change.
See **Table B5** for details about the estimated specifications.
Sources: SLID, authors' calculations.

per cent or lower than 35 per cent, since wage changes outside this interval seem implausible for job-stayers. Albeit arbitrary, these bounds are used in the literature, for instance by Dickens et al. (2007) and Deelen and Verbeek (2015). Additionally using -30 and 30 per cent cut-offs yields similar results for the effects of DNWR: of smaller magnitude in absolute values (first panel of **Table 1**), but of similar magnitude relative to average wage growth (second panel of **Table 1**). The latter restrictions can be justified by noting that the average wage change in SLID, once restricted to this interval, is very similar to the change in average wages in other Canadian data sources, such as LFS or SEPH (**Figure 1**). The estimation results are also robust to the inclusion of changes in provincial minimum wages as an explanatory variable. These

results are comparable with other findings in the literature. Fehr and Goette (2005) estimate that the average prevented wage cut was 2.7 per cent, which implies that the average wage was 0.9 pp higher. Altonji and Devereux (2000) find that the average prevented cut in the United States was 6.5 per cent, which implies that DNWR kept wage growth higher by 4 pp.

Using composite wage changes yields lower estimated effects of DNWR than when using the end-of-year wage change. This may be because composite wages are computed as average wages for each year, and so the change in composite wages over two years encompasses changes in wages in either of these two years, while the change in the end-of-year wage (December to December) covers a shorter period of time. In other words, the effects of DNWR are lower over a longer period of time (full two years) when there is more time and opportunities for wages to change. This result is consistent with the finding in Card and Hyslop (1997) that the incidence of wage freezes is a negative function of the time horizon over which wage changes are measured (more wage freezes are observed over a shorter time horizon).

Additionally, the results suggest that the effects of DNWR were much stronger during the periods of low CPI inflation in Canada, a result also found in some other countries, for instance Switzerland (Fehr and Goette, 2005) and the Netherlands (Deelen and Verbeek, 2015). Specifically, the effects of DNWR on average wage growth are more than five times larger when inflation is below 1.5 per cent than when inflation is above 2.5 per cent. This result is consistent with the observed wage-change distributions in **Figure 6**. Assuming that DNWR indeed has a real impact on labour market adjustments — and keeping in mind that our reduced-form model does not test this assumption — our results suggest that the impact of negative labour demand shocks on unemployment may be larger when inflation is low: not only are estimates of DNWR higher, but inflation takes longer to erode real wages in the absence of nominal wage cuts. Note that the effect is unlikely to be symmetric, however. This result is consistent with Crawford (2001), who shows that the incidence of wage freezes is higher during periods of low inflation.

## 4.3 Percentage of workers affected by DNWR in SLID

Another indicator of wage rigidity is the percentage of workers affected by DNWR. **Table 2** shows these fractions. Overall, the fraction of workers affected by DNWR is about 7 per cent for composite wages and 20 per cent for the end-of-year wage. Reflecting the numbers presented in **Table 1**, the percentage of workers affected by DNWR at least doubled after the Great Recession (2009 to 2011), compared with the 2001 to 2008 period. It is also interesting to note that while the effects of DNWR in **Table 1** declined when we further restricted the sample of wage changes to -30 to +30 per cent, the percentage of workers affected by DNWR did not change much, as shown in **Table 2**; this is most evident in the end-of-year wage-change results.

**Table 2: Percentage of workers affected by DNWR**

| | **Wage change [-35%;-60%]** | | **Wage change [-30%;-30%]** | |
|---|---|---|---|---|
| | EoY | Comp. | EoY | Comp. |
| All years | 19.5 | 7.3 | 19.8 | 6.2 |
| Years: 1994–2000 | 16.8 | 12.2 | 17.9 | 11.7 |
| Years: 2001–08 | 16.4 | 2.7 | 16.2 | 1.8 |
| Years: 2009–11 | 32.3 | 9.0 | 32.0 | 6.3 |
| Provincial inflation: < 1.5% | 27.3 | 13.8 | 28.7 | 12.4 |
| Provincial inflation: 1.5–2.0% | 20.2 | 6.0 | 20.2 | 4.9 |
| Provincial inflation: 2.0–2.5% | 16.2 | 5.2 | 15.8 | 4.2 |
| Provincial inflation: >2.5% | 9.7 | 2.1 | 9.3 | 1.4 |
| Unionized firms, 500+, non-supervisory | 18.8 | 6.8 | 18.5 | 5.6 |
| Unionized firms, 500+, non-supervisory: 1994–2000 | 16.6 | 12.5 | 17.2 | 11.6 |
| Unionized firms, 500+, non-supervisory: 2001–08 | 15.9 | 1.3 | 15.0 | 0.7 |
| Unionized firms, 500+, non-supervisory: 2009–11 | 30.5 | 8.2 | 29.7 | 5.3 |

EoY: End-of-year wage change; Comp.: composite wage change
See **Table B5** for details about the estimated specifications.
Sources: SLID, authors' calculations.

The proportion of workers affected by DNWR in the post-recession period, compared with the pre-recession period, increased from about 16 per cent to 32 per cent for end-of-year wage changes, but from about 2 per cent to about 8 per cent for the composite wage changes. Similarly, the proportion of workers affected by DNWR is

larger by 11–19 percentage points when inflation is low (<1.5 per cent relative to >2.5 per cent).

Our estimates are again comparable with other results found in the literature. For example, Deelen and Verbeek (2015) find that the share of workers affected by DNWR ranges between 12 and 53 per cent, using Dutch data. Fehr and Goette (2005) estimate that DNWR prevented wage cuts for one-third of job-stayers, using Swiss data. Using US data, Altonji and Devereux (2000) find that cuts were prevented for 62 per cent of job-stayers.

## 4.4 Comparison of DNWR results from MWS and SLID

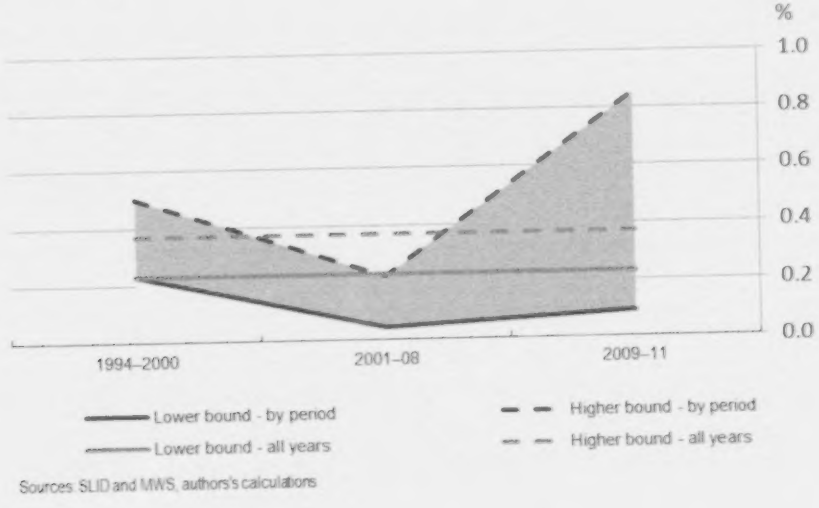
On average, between 1994 and 2011, the results suggest that the effects of DNWR added about 0.2 to 0.4 pp to wage growth based on estimates from the MWS and SLID data (**Table 1** and **Table B4**) using provincial CPI.

As pointed out in Section 2, SLID covers the entire labour force in Canada, unlike MWS. It is possible, however, to compare the results by restricting the SLID sample to individuals working in unionized firms with at least 500 employees. We also exclude workers holding supervisory jobs as these individuals are typically not members of a union. Results in the bottom panel of **Table B4** (MWS) should be compared with estimates in the bottom panel of **Table 1** (SLID). The comparison is done for estimates based on CPI provincial inflation.

**Figure 10** shows the range of the effects of DNWR on wage growth for the whole period (1994–2011) and for three subperiods: 1994–2000, 2001–08 and 2009–11. Overall, the effects of DNWR from the first-year increase (MWS) and the effects of DNWR with the end-of-year increase (SLID) range from 0.23 pp to 0.37pp over the period common to both datasets (1994 to 2011). These two measures are most comparable, as they represent wage changes over one year—keep in mind that end-of-year captures annual wage changes from December to December. The light blue area in **Figure 10**

represents the range for the three subperiods. The range decreased from 0.23–0.50 pp in 1994–2000 to 0.04–0.22 in 2001–2009, but then increased to 0.09–0.85 pp after the Great Recession.

**Figure 10: Range of the effects of DNWR on wage growth, MWS and SLID results, by period**




Sources: SLID and MWS, authors's calculations

The large increase in the range for the 2009–11 period stems from a dramatic increase in the SLID estimate and a relatively low increase in the MWS estimate. From **Table B4** and **Table 1**, it is interesting to note that the SLID results suggest that the effects of DNWR were higher in 2009–11 relative to 1994–2000, while the MWS results suggest the opposite. The overall dynamics are, nevertheless, that the effects of DNWR have increased since 2009.

Comparison of the average wage change (MWS) and composite wage change (SLID) suggest that the effects of DNWR are much smaller. Overall, the estimates range from 0.04 to 0.08 pp. Only the 2001–08 period results were found to be higher, ranging from 0.11 to 0.17 pp. But, again, the post-2009 dynamics are consistent with the results mentioned in the previous paragraph, which suggest that the effects of DNWR on wage growth have increased in recent years.

## 4.5 Heterogeneity and DNWR

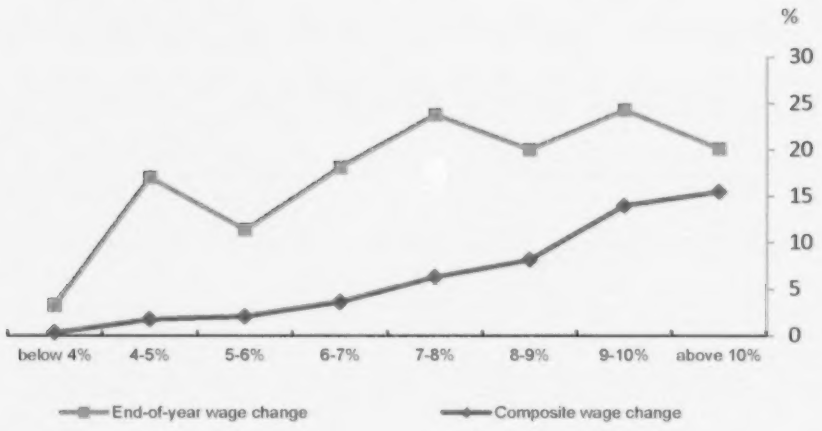
We also compute the effects of DNWR based on SLID data for different levels of unemployment rates in the province of workers' residence. **Figure 11** presents the effects of DNWR on average wage growth for different levels of unemployment rate (in pp), and **Figure 12** presents corresponding fractions of workers affected by DNWR (both figures show results from the end-of-year wage-change estimations). These figures illustrate that higher unemployment rates are associated with larger effects of DNWR on average wage growth and with a higher fraction of workers affected by DNWR. While our reduced-form model does not permit us to establish any form of causality, the evidence seems to be broadly consistent with expectations about the presence of DNWR and unemployment rates.

**Figure 11: Measure of DNWR by unemployment rate**

Sources: SLID, authors' calculation

We also find that DNWR does not affect all groups of the population to the same extent, as detailed in **Table B6** and **Table B7** (showing the effects of DNWR on average wage growth and the percentage of workers affected by DNWR, respectively). In particular, the estimation results suggest that the effects of DNWR are more pronounced for the following groups of workers: older (55+), low education and low-paid workers, employed in the private sector, employed in goods-producing industries

**Figure 12: Fraction of workers affected by DNWR by unemployment rate**




Sources: SLID, authors' calculation

(with mining, oil and gas being an exception and demonstrating much lower DNWR effects), unionized (for firms with more than 500 employees), working in smaller firms, employed in non-supervisory occupations, and immigrants. Overall, population ageing and an increasing proportion of immigrants may continue to increase the effects of DNWR in Canada, while the continuing shift towards service industries and the increasing educational attainment of the Canadian population might reduce them.

It is interesting to compare some of these results with what is found in the literature for other countries. For example, we find that the effect of DNWR is larger for workers with low levels of education, in contrast to what was found by Deelen and Verbeek (2015). Differences in wage definition could explain this discrepancy: our wage measure includes tips, commissions and bonuses, and is thus closer to total compensation than to a base pay, as in Deelen and Verbeek (2015). This would also be consistent with previous results from Crawford and Harrison (1998) showing that total compensation exhibits less rigidity than base pay. Interestingly, using total compensation (or something close to this concept) could also explain why workers with higher education experienced less rigidity, as they are more likely to get bonuses

and other additional payments than less-educated workers. We also found that the estimated effects of DNWR are larger for low-paid workers. Assuming that low-paid workers are more likely to occupy an hourly paid job, our finding is consistent with Daly, Hobijn and Lucking (2012) who find that the share of wage freezes is higher for hourly workers than for non-hourly workers.

Finally, we find higher effects of DNWR for workers in unionized firms with more than 500 employees relative to non-unionized firms of a similar size. This result is consistent with Holden and Wulfsberg (2008), who find that higher nominal wage rigidity is more prevalent when union density is high (proportion of paid workers who are union members). We, however, find heterogeneity in the effect of unionization by firm size. In particular, the effects of DNWR are higher for large unionized firms, but they are lower for all unionized firms. The latter result is consistent with Kahn (1997) who finds that the incidence of wage freezes is lower for unionized workers. In our data, this heterogeneity is driven by the firm size, because we find that the effects of DNWR are significantly larger for smaller firms (which are in their turn significantly less likely to be unionized than firms with more than 500 employees).

## 4.6 Caveats

At this point, three observations are warranted. First, as discussed above, SLID contains self-reported data on wages, which may be subject to reporting error. Despite all the precautionary measures taken in this study to minimize the impact of such errors, we could not guarantee that the results using SLID data are not at all affected. However, we see two major advantages of enriching our analysis using SLID data: i) SLID provides evidence of the total wage compensation (versus base pay in MWS), which may be more important for a firm's decision, ii) SLID allows us to explore heterogeneity in the effects of DNWR and to extend conclusions to the entire labour market rather than limiting them to large unionized firms. Additionally, we may realistically assume that if reporting errors remain in the data, they are identically distributed across time and different subgroups of the population. We thus can rely

on the results in terms of their dynamics over time, over periods with different inflation levels, and across observable characteristics. It is nevertheless reassuring that although the estimated effects of DNWR are higher using SLID data, both MWS and SLID data suggest similar dynamics over the analyzed time period, for example, that the effects of DNWR increased following the Great Recession.

Validation studies such as Bound and Krueger (1991) and Bound et al. (1994) look into measurement errors in survey data. For example, Bound and Krueger (1991) compared matched Current Population Survey with administrative Social Security payroll tax records and concluded that "longitudinal data are more reliable than previously believed" [Bound and Krueger (1991), p.1].[17] Altonji and Devereux (2000) account for measurement error in PSID data and find that the frequency of nominal wage cuts is lower and the frequency of wage freezes is higher after correcting for measurement error. They find that, essentially, almost all of the wage cuts are due to measurement error. Gottschalk (2005) uses an approach based on methods for estimating structural breaks in time series to construct micro-level adjusted wage series and finds that correcting for measurement error eliminates a substantial number of wage cuts: from 17 per cent to about 4–5 per cent in PSID and the Survey of Income and Program Participation (SIPP). Thus, true downward flexibility of wages is lower than downward flexibility in reported wages. Dickens et al. (2007) find that measurement error leads to downward bias in measures of nominal wage rigidity. To summarize, these studies point out that measurement error overstates downward flexibility and understates wage rigidity. We can approach our findings from SLID as potentially downwardly biased measures of DNWR if reporting errors in SLID are of the same nature and magnitude as in the studies discussed above.

Second, one main difference with Crawford and Wright (2001) is that we included institutions in both the private and public sectors while they only used agreements signed in the private sector. We included the public sector to be consistent with the

[17] Bound and Krueger (1991) found that ratios of variance of signal to variance of noise are quite high: 0.82 for men and 0.92 for women.

SLID analysis. The main motivation for excluding the public sector is that the wage settlement dynamics may be different because of the high unionization rate and the small share of public sector employment in the economy, which means that the public sector is overrepresented in the MWS data. However, while the peak impact after recessions seems mainly driven by an increase in wage freezes in the public sector, there is no clear difference between sectors in other periods. Similar conclusions are reached when using the average wage-change measure.

Third, how can the MWS results inform us on the extent of DNWR in the whole economy? Estimates based on SLID data suggest higher effects of DNWR than those based on MWS data. The main explanation is that MWS covers only subgroups of the employed population. Additionally, any comparison between the two sets of estimates must be done with caution. One reason is that wage dynamics are different: the fraction of workers with observed wage cuts is much higher in SLID than in MWS, which led us to develop alternative DNWR measures for the SLID data. The second reason is that the observational unit in the MWS data is an employer, while it is a worker in SLID. We also used a longer horizon (about three years ahead) for inflation expectations variables with the MWS data, as the average duration of agreements is about 30 months. It is however reassuring that estimations based on both data sources suggest similar dynamics in the effects of DNWR over time, in particular that it is increasing after recessions.

Finally, the estimations of DNWR in this paper were conducted for job-stayers only. What can we infer for the entire employed population from these results? Over time we observe a decline in the share of new hires — defined as those with job tenure of 1 to 12 months — in total employment from a peak of 28 per cent in the late 1980s to 20 per cent last year (LFS data). As the wages of new hires are more flexible, the decline in the share of new hires implies more DNWR in the economy, all else being equal. Therefore, assuming as a lower bound a zero effect of DNWR for new hires, the estimation results based on both data sources could be adjusted downward by up to 20 per cent in order to have an approximate impact for the whole economy, including

new hires.[18] The impact of DNWR likely increased after 2011 up to 2013, but then decreased, as suggested by the most recent MWS data for large unionized firms. It is, however, difficult to generalize this result to the entire population of workers in Canada.

# 5 Conclusion

In this paper, we assess the importance of DNWR in Canada. Using SLID data, our results show that the average, median and variance of the wage-changes distribution decreased over time. We also observe a substantial increase in the proportion of workers experiencing wage freezes since 2004, along with a declining pattern in both wage cuts and increases. The latter stylized fact would be consistent with a higher impact of DNWR in recent years compared with the 1990s and early 2000s.

We empirically estimate the underlying notional wage distribution and calculate the effect of DNWR on average wage growth and the proportion of workers affected by DNWR. We find evidence of DNWR in both the SLID and MWS data. The impact of DNWR on average wage growth varies depending on the data source and wage measure. Based on both data sources, overall for the total economy, we find that the estimated impact of DNWR on average wage growth increased in the years following the 2009 recession: average wage growth in the economy was higher due to DNWR for the 2009 to 2011 period, ranging from 0.09 to 0.95 pp. Likewise, the proportion of workers affected by DNWR increased from around 16 per cent to up to 32 per cent after the Great Recession (comparing 2001–08 with 2009–11, as suggested by the SLID data). Moreover, the impact of DNWR likely increased post-2011 up to 2013, but decreased after, as suggested by the MWS data.

Additionally, we find that the effects of DNWR are much lower based on composite

[18] Further investigation, which is beyond the scope of our paper, is needed to evaluate the wage flexibility of new hires. For now, we rely on the findings in the literature regarding the wage dynamics of new hires (see Haefke, Sonntag and van Rens 2013).

wage changes than its effects based on the end-of-year changes. In other words, the effects of DNWR are higher for wage changes computed based on the information collected over a shorter period of time (one year) than those computed using information over a longer period of time (two years).

We also find evidence that higher effects of DNWR are associated with lower inflation and higher unemployment based on provincial inflation rates and unemployment rates. Indeed, the effect of DNWR on average wage growth was much stronger during periods of low CPI inflation in Canada: the effect on average wage growth was more than five times larger when inflation was below 1.5 per cent compared with periods when inflation was above 2.5 per cent; as well, the percentage of workers affected by DNWR was larger by 11–19 percentage points during the periods of lower inflation. Finally, the paper provides an extensive analysis of the heterogeneity in the effects of DNWR. This analysis suggests, for instance, that population ageing and an increasing proportion of immigrants may continue to increase the effect of DNWR in Canada, while the continuing shift towards service industries and increasing educational attainment of the Canadian population might reduce them.

One direction for future work could be to relax the assumption that the notional wage distribution is normal. The normality assumption may be questionable, as the resulting predicted wage-change distribution from the Tobit does not replicate well the long tails of the observed distribution of wage changes. That could happen because the normal distribution does not have tails that are fat enough. Since the notional wage distribution is based on the same estimated parameters from the Tobit, there is a risk that DNWR effects may be over- or underestimated. Future work could thus focus on trying other distributional assumptions in the model, for example, the Weibull distribution as in Deelen and Verbeek (2015). A second interesting direction for future research that we are also going to undertake is to make better use of the long-term panel dimension of the SLID by introducing individual effects or looking at long-term job-stayers, for instance. More research should also be devoted to studying why the effects of DNWR affect some groups more than others. This may have some

macroeconomic consequences, as the composition of the working-age population is expected to change in coming years due to factors such as the ageing population and immigration, among others.

The last, but not the least, natural extension of our work would be to assess the impact of DNWR on the long-term unemployment rate in Canada. We should be cautious before concluding that our results imply that DNWR has real macroeconomic effects on, for example, long-run employment. This is especially true considering that the analysis excludes other types of labour market frictions that could affect unemployment in the absence of DNWR. Some previous studies analyzing DNWR in Canada (e.g., Fortin 2013 and Simpson, Cameron and Hum 1998) find that the combination of DNWR and low inflation pushed unemployment above the level at which it would have been in the absence of DNWR, suggesting that real wage erosion through inflation takes more time when inflation is low. In contrast, Farès and Lemieux (2000), Faruqui (2000) and Farès and Hogan (2000) find that DNWR had no long-term effect on unemployment. Overall, the presence of DNWR is not by itself sufficient to support the inference that the natural rate of unemployment may be higher than it would be in the absence of DNWR.

# References


Akerlof, G. A., W. T. Dickens and G. L. Perry. 1996. "The Macroeconomics of Low Inflation." Brookings Papers on Economic Activity, 1.

Altonji, J. G. and P. J. Devereux. 2000."The Extent and Consequences of Downward Nominal Wage Rigidity." *Worker Well-Being*, Research in Labor Economics, vol. 19, pp. 383–431

Amirault, D., P. Fenton and T. Laflèche. 2013. "Asking About Wages: Results from the Bank of Canadas Wage Setting Survey of Canadian Companies." Bank of Canada Discussion Paper 2013–1.

Babecký, J., P. Du Caju, T. Kosma, M. Lawless, J. Messina and T. Room. 2009. "Downward Nominal And Real Wage Rigidity: Survey Evidence From European Firms." European Central Bank Working Paper Series 1105.

Bound, J. and A. B. Krueger. 1991. "The Extent of Measurement Error in Longitudinal Earnings Data: Do Two Wrongs Make a Right?" *Journal of Labor Economics*, (9)1, pp. 1–24.

Bound, J., C. Brown, G. Duncan, and W. Rodgers. 1994. "Evidence on the Validity of Cross-Sectional and Longitudinal Labor Market Data." *Journal of Labor Economics*, vol. 2(2), pp. 345–368.

Card, D., and D. Hyslop. 1997. Does Inflation "Grease the Wheels of the Labor Market?" In *Reducing Inflation: Motivation and Strategy*, ed. Christina D. Romer and David H. Romer, pp. 71–122

Crawford, A. 2001. "How Rigid Are Nominal-Wage Rates?" Bank of Canada Working Paper 2001–8.

Crawford, A. and A. Harrison. 1998. "Testing for Downward Rigidity in Nominal Wage Rates." In *Price Stability, Inflation Targets, and Monetary Policy*, pp. 179–218. Proceedings of a conference held by the Bank of Canada, May 1997. Ottawa: Bank of Canada.

Crawford, A. and G. Wright. 2001. "Downward Nominal-Wage Rigidity: Micro Evidence from Tobit Models." Bank of Canada Working Paper 2001–7.

Daly, M., B. Hobijn and B. Lucking. 2012. "Why Has Wage Growth Stayed Strong?"

Federal Reserve Board of San Francisco Economic Letter, 2012–10.

Daly, M. and B. Hobijn. 2014. "Downward Nominal Wage Rigidities Bend the Phillips Curve?" Federal Reserve Board of San Francisco Working Paper 2013–08.

Deelen, A. and W. Verbeek. 2015. "Measuring Downward Nominal and Real Wage Rigidity – Why Methods Matter." Manuscript.

Dickens, W. T., L. Goette, E. L. Groshen, S. Holden, J. Messina, M. E. Schweitzer, J. Turunen and M. Ward. 2007. "How Wages Change: Micro Evidence from the International Wage Flexibility Project." *Journal of Economic Perspectives*, vol. 21(2), pp. 195–214.

Fallick, B., Lettau, M., and W. Wascher. 2016. "Downward Nominal Wage Rigidity in the United States during and after the Great Recession." Federal Reserve Bank of Cleveland Working Paper 2016–02.

Farès, J. and S. Hogan. 2000. "The Employment Costs of Downward Nominal-Wage Rigidity." Bank of Canada Staff Working Paper 2000–1.

Farès, J. and T. Lemieux. 2000. "Downward Nominal-Wage Rigidity: A Critical Assessment and Some New Evidence for Canada." In *Price Stability and the Long-Run Target for Monetary Policy*, pp. 3–31. Proceedings of a conference held by the Bank of Canada, June 2000. Ottawa: Bank of Canada.

Faruqui, U. 2000. "Employment Effects of Nominal-Wage Rigidity: An Examination Using Wage-Settlements Data." Bank of Canada Working Paper 2000–14.

Fehr, E. and L. Goette. 2005. "Robustness and Real Consequences of Nominal Wage Rigidity." *Journal of Monetary Economics*, vol. 52, pp. 779–804.

Fortin, P. 2013. "The Macroeconomics of Downward Nominal Wage Rigidity: a Review of the Issues and New Evidence for Canada." Centre Interuniversitaire sur le Risque, les Politiques conomiques et lEmploi, Working Paper 13–09.

Gauthier-Loiselle, M. 2013. "Three essays in public policies: Unemployment insurance, parental leave, and breastfeeding subsidy." Department of Economics. Princeton, NJ: Princeton University.

Gottschalk, P. 2005. "Downward nominal-wage flexibility: real or measurement er-

ror." *The Review of Economics and Statistics*, vol. 87(3), pp. 556–568.

Gray, D., and Qiu, H. 2010. "The responsiveness of industry wages to low-frequency shocks in Canada." *Canadian Journal of Economics*, vol. 43(4), pp. 1221–1242.

Haefke, C., M. Sonntag, and T. van Rens. 2013. "Wage rigidity and job creation." *Journal of Monetary Economics*, vol. 60(6), pp. 887–899.

Holden, S., and F. Wulfsberg. 2008. "Downward Nominal Wage Rigidity in the OECD." Manuscript.

Hum, D., Simpson, W. 2004. "Reinterpreting the performance of immigrant wages from panel data." *Empirical Economics*, vol. 29(1), pp. 129–147.

Kahn, S. 1997. "Evidence of Nominal Wage Stickiness from Microdata." *The American Economic Review*, vol. 87(5), pp. 993–1008.

Khan, M., L. Morel and P. Sabourin. 2013. "The Common Component of CPI: An Alternative Measure of Underlying Inflation for Canada." Bank of Canada Working Paper 2013–35.

Lebow, D. E., Saks, R. E. and B.A. Wilson. 1999. "Downward Nominal Wage Rigidity: Evidence from the Employment Cost Index." Finance and Economics Discussion Series 1999–31. Board of Governors of the Federal Reserve System (US).

McLaughlin, K. J. 1994. "Rigid Wages?" *Journal of Monetary Economics*, vol. 34(3), pp. 383–414.

Nelson, F. 1977. "Censored Regression Models with Unobserved, Stochastic Censoring Thresholds." *Journal of Econometrics*, vol. 6, pp. 309–327.

Robertson, J. 2015. "No wage change?" Federal Reserve Bank of Atlanta, Aug. 21, 2015. Available at http://macroblog.typepad.com/macroblog/2015/08/no-wage-change.html

Schirle, T. 2012. "Wage Losses of Displaced Older Men: Does Selective Retirement Bias Results?" *Canadian Public Policy*, vol. 38(1), pp. 1–13.

Simpson, W., N. Cameron and D. Hum (1998). "Is Hypoinflation Good Policy?" *Canadian Public Policy*, vol. 24(3), pp. 291–309.

Tobin, J. 1972. "Inflation and Unemployment," *American Economic Review*, vol. 62, pp. 1–18.

Yoshida, Y., Smith M. R. 2005. "Training and the earnings of immigrant males: Evidence from the Canadian Workplace and Employee Survey." *Social Science Quarterly*, vol. 86(S1), pp. 1218–1241

Yoshida, Y., Smith, M.R. 2008. "Measuring and Mismeasuring Discrimination Against Visible Minority Immigrants: The role of work experience." *Canadian Studies in Population*, vol. 35(2), pp. 311–338.

# Appendix A

In addition to self-reported data, SLID also contains data on earnings linked to tax-file information. This measure includes earnings reported in a T4 slip, an official form submitted by all employers to the Canada Revenue Agency, as well as other employment income from the same employer, including research grants, tips, gratuities or directors fees not reported on the T4. Total earnings reported in the tax declaration are gross earnings from all jobs held as an employee, before payroll deductions such as income taxes, employment insurance contributions or pension plan contributions, etc. Wages and salaries also include the earnings of owners of incorporated businesses, although some amounts may instead be reported as investment income. Other components included in total employment income are commission income and overtime pay, taxable benefits, taxable allowances, and honorariums, as well as employer-paid maternity and parental benefits, among other occasional payments.

Therefore, total earnings derived from the tax files encompass the definition of wages self-reported in SLID, but also include many other payments received by the employee from the employer. The data suggest that changes in the earnings per hour worked derived from the tax-based measure of total earnings and hours worked, both usual and actual, are significantly more volatile than self-reported hourly earnings. As a result, the distribution of changes in hourly earnings based on tax-file information is almost symmetric around zero (see **Figure A1**). The spike at zero is significantly less pronounced relative to the spike in the changes in self-reported wages. The left tail of the distribution of changes in hourly earnings is almost as large as the right tail. These data thus suggest that the earnings supplemental to the base pay are less rigid, and are also less downwardly rigid, and might be used by employers to adjust the overall costs of labour.

**Figure A1: Distribution of changes in hourly earnings based on tax-file information, 1994-2011.**

Sources: SLID, authors' calculations

# Appendix B

**Table B1: Job-stayers differ from the total population of workers**

| Descriptive statistics | All employed, in December | Job-stayers |
|---|---|---|
| Males, % | 51.8% | 53.0% |
| Age (average) | 39.08 | 42.65 |
| 15–24, % | 12.8% | 3.5% |
| 25–54, % | 76.5% | 83.2% |
| 55–69, % | 10.6% | 13.2% |
| Higher education, % | 20.6% | 21.6% |
| Post–secondary education, % | 49.7% | 50.2% |
| High school or below, % | 29.7% | 28.2% |
| Private sector, % | 75.7% | 70.1% |
| Ontario, % | 38.4% | 37.1% |
| Quebec, % | 25.4% | 27.9% |
| British Columbia, % | 12.2% | 11.4% |
| Prairies, % | 16.7% | 16.0% |
| Atlantic, % | 7.3% | 7.6% |
| Rural areas, % | 9.9% | 9.9% |
| Below 29 K, % | 10.5% | 10.7% |
| 30–99 K, % | 10.0% | 10.4% |
| 100–499 K, % | 18.6% | 19.2% |
| More than 500 K, % | 51.1% | 49.8% |
| Full–time job | 83.8% | 88.7% |
| Service industries, % | 75.9% | 74.9% |
| Goods industries, % | 24.1% | 25.1% |
| Mining, oil and gas (MOG), % | 1.5% | 1.5% |
| Manufacturing (MAN), % | 15.2% | 17.5% |
| Goods industries other than MOG and MAN, % | 7.5% | 6.2% |
| Unionized, % | 32.3% | 39.8% |
| Covered by a collective agreement, % | 34.8% | 42.5% |
| Firm size: 0–19 | 20.6% | 15.2% |
| Firm size: 20–99 | 16.8% | 15.5% |
| Firm size: 100–499 | 14.6% | 15.4% |
| Firm size: 500–999 | 7.8% | 8.7% |
| Firm size: more than 1,000 | 40.1% | 45.2% |
| Have supervisor responsibilities, % | 20.7% | 23.8% |
| Enrolled in educational establishments, % | 15.0% | 7.3% |
| Aboriginal, % | 3.3% | 2.7% |
| Visible minority, % | 11.7% | 9.5% |
| Immigrant, % | 16.8% | 16.0% |
| Years since immigration to Canada | 3.44 | 3.78 |

Source: SLID, authors' calculations.

**Table B2: Inflation, inflation expectations and wage growth**

| | 1994–2000 | 2001–2008 | 2009–2011 |
|---|---|---|---|
| **Inflation** | | | |
| CPI | 1.57 | 2.27 | 1.66 |
| Common component | 1.49 | 2.03 | 1.96 |
| **Survey inflation expectations** | | | |
| Towers-Watson | 1.89 | 2.14 | 1.70 |
| Conference Board | 1.90 | 2.03 | 1.75 |
| Consensus Economics | 1.78 | 2.01 | 1.77 |
| Finance Canada | 1.90 | 2.03 | 1.75 |
| **Market-based** | | | |
| FMD | 2.00 | 1.81 | 1.84 |

Sources: Statistics Canada, Towers Watson, Conference Board, Consensus Economics, Finance Canada, Bank of Canada, authors' calculations.

**Table B3: Selected estimated coefficients from the TOBIT model (MWS data)**

| | First year wage change | | Average change over contract duration | |
|---|---|---|---|---|
| Unemployment rate | −0.073 | 0.022 | −0.021 | 0.079 |
| | (0.071) | (0.115) | (0.046) | (0.066) |
| Firm size: 1001 to 2000 (= 1) | −0.063 | −0.028 | −0.055* | −0.033 |
| | (0.048) | (0.057) | (0.032) | (0.036) |
| Firm size: 2001 to 5000 (= 1) | −0.168*** | −0.088 | −0.091** | −0.046 |
| | (0.055) | (0.065) | (0.036) | (0.041) |
| Firm size: 5001+ (= 1) | −0.220*** | −0.198** | −0.160*** | −0.152*** |
| | (0.068) | (0.081) | (0.041) | (0.046) |
| Cost of living clause (= 1) | 0.218*** | 0.064 | 0.189*** | 0.069 |
| | (0.058) | (0.100) | (0.038) | (0.057) |
| Public sector (= 1) | −0.170*** | −0.223*** | −0.110*** | −0.100** |
| | (0.058) | (0.070) | (0.036) | (0.042) |
| CPI by province | 0.263*** | | 0.176*** | |
| | (0.023) | | (0.016) | |
| Consensus Economics | | 0.913* | | 0.639** |
| | | (0.482) | | (0.273) |
| Constant | 2.353*** | 0.712 | 2.433*** | 1.013 |
| | (0.508) | (1.174) | (0.333) | (0.700) |
| $k_0$ | 0.825*** | 0.812*** | 0.470*** | 0.406*** |
| | (0.028) | (0.030) | (0.019) | (0.022) |
| sqrt(log($\sigma_u$)) | −0.762*** | −0.812*** | −1.199*** | −1.415*** |
| | (0.044) | (0.045) | (0.072) | (0.112) |
| $\alpha_1$ | −0.001*** | −0.002*** | −0.002*** | −0.002*** |
| | (0.000) | (0.001) | (0.000) | (0.000) |
| $\alpha_0$ | 1.049*** | 1.407*** | 0.801*** | 0.818*** |
| | (0.041) | (0.236) | (0.028) | (0.092) |

Notes: Estimated coefficients are for the model with menu-cost and time-dependent variance. Regressions also include binary variables for year and region. (= 1) means the variable is binary and equals 1 if the statement is true. The estimated coefficients for the unemployment rate become negative and significant if the year binary variables are not included close to -0.3, consistent with the results from Crawford and Wright (2001).
Standard errors in parentheses: *** $p < 0.01$,
** $p < 0.05$, * $p < 0.1$.
Sources: MWS, authors' calculations.

Table B4: Selected estimated effects of DNWR on wage growth (percentage points), MWS, by period

| | | CPI | CPI by province | Finance Canada | Consensus Economics |
|---|---|---|---|---|---|
| | 1978–1991 | 0.04 | 0.03 | | |
| **First year** | 1992–2000 | 0.58 | 0.50 | 0.64 | 0.51 |
| **wage** | 2001–2008 | 0.04 | 0.04 | 0.04 | 0.04 |
| **change** | 2009–2015 | 0.12 | 0.15 | 0.12 | 0.12 |
| | All years | 0.18 | 0.19 | 0.26 | 0.20 |
| **Average** | 1978–1991 | 0.01 | 0.00 | | |
| **change** | 1992–2000 | 0.21 | 0.18 | 0.18 | 0.13 |
| **over** | 2001–2008 | 0.00 | 0.00 | 0.00 | 0.00 |
| **contract** | 2009–2015 | 0.01 | 0.02 | 0.02 | 0.02 |
| **duration** | All years | 0.06 | 0.06 | 0.07 | 0.05 |
| *Periods consistent with SLID* | | | | | |
| **First year** | 1994–2000 | 0.57 | 0.50 | 0.64 | 0.51 |
| **wage** | 2001–2008 | 0.04 | 0.04 | 0.04 | 0.04 |
| **change** | 2009–2011 | 0.07 | 0.09 | 0.08 | 0.08 |
| | All years | 0.25 | 0.23 | 0.28 | 0.22 |
| **Average** | 1994–2000 | 0.21 | 0.17 | 0.18 | 0.13 |
| **change over** | 2001–2008 | 0.00 | 0.00 | 0.00 | 0.00 |
| **contract** | 2009–2011 | 0.01 | 0.01 | 0.01 | 0.01 |
| **duration** | All years | 0.08 | 0.07 | 0.07 | 0.05 |

Note: The estimated coefficients used are from the model estimated with menu-cost and time-dependent variance. The effects of DNWR in the table, however, do not include the effect of menu-cost and, therefore, represent the pure effect of DNWR. Averaging over models (simple Tobit, Tobit with time-dependent variance, and menu-costs + time dependent variance) yields a similar dynamic, but slightly larger DNWR effects. Not all series start in 1978, which explains the empty cells for some periods.
Sources: MWS, authors' calculations.

**Table B5: Estimated coefficients from the TOBIT model (SLID data)**

| | **Wage change [-35%;60%]** | | **Wage change [-30%;30%]** | |
|---|---|---|---|---|
| | EoY | Comp. | EoY | Comp. |
| Provincial CPI inflation | 0.7993*** | 0.3781*** | 0.5979*** | 0.3704*** |
| Provincial UR | 0.4663*** | 0.4952*** | 0.2750*** | 0.3370*** |
| Provincial GDP, log | 0.4560*** | 0.4466*** | 0.1892* | 0.3151*** |
| Male | 0.0573 | 0.0828 | 0.3649*** | 0.1748* |
| Age: 15-24 | 6.2586*** | 4.8642*** | 4.0771*** | 3.3626*** |
| Age: 25-54 | 2.0103*** | 1.6016*** | 1.4017*** | 1.2479*** |
| Higher Education Degree | 3.0204*** | 2.3401*** | 2.1410*** | 1.7104*** |
| Post-Sec. Education Degree | 0.7610*** | 0.8037*** | 0.6449*** | 0.5980*** |
| Industry: MOG | 0.5435 | 0.6209 | 0.2887 | 0.0361 |
| Industry: MAN | 0.3426 | 0.2089 | 0.1755 | 0.0677 |
| Industry: Other Goods | 0.5554* | 0.5915** | 0.0842 | 0.3053* |
| Public Sector | 0.3396 | 0.0517 | 0.1889 | 0.0774 |
| Firm size: 1-19 | 1.9715*** | 1.6781*** | 1.5324*** | 1.3870*** |
| Firm size: 20-99 | 0.8781*** | 0.8103*** | 0.5649*** | 0.5191*** |
| Firm size: 100-499 | 0.0461 | 0.1968 | 0.1526 | 0.1510 |
| Firm size: 500-999 | 0.2298 | 0.1426 | 0.0295 | 0.0376 |
| Full-time work | 0.2368 | 0.1045 | 0.6528*** | 0.4130*** |
| Union | 0.0084 | 0.2409* | 0.1225 | 0.2415** |
| Supervisory role | 1.6595*** | 1.4848*** | 0.7898*** | 0.8853*** |
| $k_0$ | 0.0390*** | 0.0358*** | 0.0433*** | 0.0396*** |
| $ln(\sigma_v) : \alpha_0$ | 2.9349*** | 2.8705*** | 2.4901*** | 2.4280*** |
| $ln(\sigma_v) : \alpha_1$ | 0.0098*** | 0.0274*** | 0.0053*** | 0.0185*** |
| $ln(\sigma_u)$ | 5.0534*** | 4.8988*** | 4.7821*** | 4.7392*** |
| Number of observations | 133,766 | 135,364 | 126,078 | 129,761 |

EoY: End-of-year wage change; Comp.: composite wage change.
Notes: aboriginal, immigrant and visible minority dummy variables, student dummy variable, linear time-trend, provincial dummies, and dummy variable for the size of the area of residence. Estimations were weighted using SLID weights developed by Statistics Canada. Listed number of observations is not weighted.
*** p<0.01, ** p<0.05, * p<0.1
Sources: SLID, authors' calculations.

**Table B6: Estimated impact of DNWR on wage growth (pp), by worker characteristics**

| | Wage change [-35%;60%] | | Wage change [-30%;30%] | |
|---|---|---|---|---|
| | EoY | Comp. | EoY | Comp. |
| All years | 0.44 | 0.1 | 0.3 | 0.06 |
| Males | 0.44 | 0.1 | 0.32 | 0.06 |
| Females | 0.45 | 0.1 | 0.28 | 0.06 |
| Age 15–24 | 0.01 | 0.00 | 0.01 | 0.00 |
| Age 25–54 | 0.34 | 0.07 | 0.23 | 0.04 |
| Age 55+ | 1.17 | 0.30 | 0.83 | 0.19 |
| Higher Education | 0.07 | 0.01 | 0.05 | 0.00 |
| Post–Secondary Education | 0.42 | 0.07 | 0.27 | 0.04 |
| High School or below | 0.76 | 0.22 | 0.54 | 0.14 |
| Public Sector | 0.32 | 0.06 | 0.20 | 0.04 |
| Private Sector | 0.49 | 0.11 | 0.34 | 0.07 |
| ON | 0.50 | 0.11 | 0.35 | 0.08 |
| QC | 0.39 | 0.09 | 0.28 | 0.06 |
| BC | 0.69 | 0.13 | 0.42 | 0.06 |
| Prairies | 0.22 | 0.02 | 0.16 | 0.01 |
| Atlantic | 0.44 | 0.12 | 0.26 | 0.07 |
| Full–time | 0.43 | 0.09 | 0.28 | 0.05 |
| Part–time | 0.52 | 0.13 | 0.50 | 0.11 |
| Service industries | 0.41 | 0.09 | 0.27 | 0.06 |
| Goods–producing industries | 0.52 | 0.12 | 0.38 | 0.07 |
| Mining, oil and gas | 0.19 | 0.03 | 0.23 | 0.03 |
| Manufacturing | 0.58 | 0.14 | 0.38 | 0.08 |
| Goods–producing, excl. MOG, MAN | 0.45 | 0.08 | 0.41 | 0.07 |
| Firm size: 0–19 | 0.97 | 0.28 | 0.75 | 0.19 |
| Firm size: 20–99 | 0.53 | 0.12 | 0.35 | 0.07 |
| Firm size: 100–499 | 0.31 | 0.04 | 0.18 | 0.02 |
| Firm size: 500–999 | 0.35 | 0.08 | 0.21 | 0.04 |
| Firm size: 1000+ | 0.29 | 0.05 | 0.19 | 0.03 |
| Firm size 500+, Unionized | 0.34 | 0.07 | 0.23 | 0.04 |
| Firm size 500+, Non–unionized | 0.26 | 0.04 | 0.16 | 0.02 |
| Supervisory responsibilities | 0.17 | 0.02 | 0.15 | 0.01 |
| No supervisory responsibilities | 0.52 | 0.12 | 0.35 | 0.08 |
| Immigrant | 0.70 | 0.16 | 0.46 | 0.10 |
| Canadian-born | 0.39 | 0.09 | 0.27 | 0.05 |
| Wage $\leq$ Minimum wage (MINW) | 0.80 | 0.22 | 0.57 | 0.13 |
| MINW*1<Wage$\leq$MINW*2 | 0.71 | 0.17 | 0.48 | 0.11 |
| MINW*2<Wage$\leq$MINW*3 | 0.50 | 0.10 | 0.33 | 0.06 |
| MINW*3<Wage$\leq$MINW*4 | 0.30 | 0.06 | 0.21 | 0.04 |
| MINW*4<Wage$\leq$MINW*5 | 0.19 | 0.04 | 0.14 | 0.03 |
| Wage>MINW*5 | 0.12 | 0.02 | 0.10 | 0.01 |

EoY: End-of-year wage change; Comp.: composite wage change.
Sources: SLID, authors' calculations.

**Table B7: Percentage of workers affected by DNWR (per cent), by worker characteristics**

| | Wage change [-35%;60%] | | Wage change [-30%;30%] | |
|---|---|---|---|---|
| | EoY | Comp. | EoY | Comp. |
| All years | 19.5% | 7.3% | 19.8% | 6.2% |
| Males | 19.4% | 7.3% | 20.8% | 6.6% |
| Females | 19.6% | 7.3% | 18.7% | 5.8% |
| Age 15–24 | 0.8% | 0.2% | 1.1% | 0.3% |
| Age 25–54 | 18.2% | 6.1% | 18.2% | 5.0% |
| Age 55+ | 32.9% | 17.2% | 34.6% | 15.1% |
| Higher Education | 5.7% | 0.9% | 5.5% | 0.8% |
| Post–Secondary Education | 20.8% | 6.2% | 20.4% | 5.0% |
| High School or below | 27.7% | 14.2% | 29.4% | 12.5% |
| Public Sector | 15.8% | 5.1% | 15.4% | 4.3% |
| Private Sector | 21.1% | 8.3% | 21.6% | 7.0% |
| ON | 20.8% | 8.6% | 21.6% | 7.9% |
| QC | 19.4% | 7.1% | 19.6% | 5.9% |
| BC | 26.0% | 10.2% | 25.7% | 7.4% |
| Prairies | 12.4% | 2.7% | 12.7% | 2.1% |
| Atlantic | 18.8% | 7.2% | 17.5% | 5.9% |
| Full–time | 19.2% | 7.1% | 19.1% | 5.7% |
| Part–time | 21.5% | 9.3% | 25.4% | 10.1% |
| Service industries | 18.4% | 6.8% | 18.4% | 5.7% |
| Goods–producing industries | 22.6% | 8.8% | 23.7% | 7.7% |
| Mining, oil and gas | 10.9% | 3.0% | 16.2% | 3.9% |
| Manufacturing | 24.0% | 9.9% | 23.9% | 8.1% |
| Goods–producing, excl. MOG, MAN | 21.6% | 7.0% | 24.9% | 7.2% |
| Firm size: 0–19 | 32.1% | 16.9% | 34.6% | 15.9% |
| Firm size: 20–99 | 22.7% | 8.9% | 23.2% | 7.0% |
| Firm size: 100–499 | 16.7% | 4.3% | 15.7% | 3.3% |
| Firm size: 500–999 | 16.7% | 6.1% | 16.0% | 5.0% |
| Firm size: 1000+ | 15.7% | 4.9% | 15.8% | 3.9% |
| Firm size 500+, Unionized | 17.7% | 6.3% | 17.6% | 5.3% |
| Firm size 500+, Non–unionized | 13.9% | 3.7% | 13.9% | 2.9% |
| Supervisory responsibilities | 10.9% | 2.1% | 12.7% | 2.2% |
| No supervisory responsibilities | 22.2% | 8.9% | 21.9% | 7.4% |
| Immigrant | 24.2% | 10.8% | 24.8% | 9.5% |
| Canadian–born | 18.6% | 6.7% | 18.8% | 5.6% |
| Wage $\leq$ Minimum wage (MINW) | 26.1% | 14.1% | 27.7% | 12.5% |
| MINW*1<Wage$\leq$MINW*2 | 27.1% | 11.7% | 27.2% | 9.7% |
| MINW*2<Wage$\leq$MINW*3 | 22.7% | 8.2% | 22.3% | 6.8% |
| MINW*3<Wage$\leq$MINW*4 | 15.8% | 5.5% | 16.3% | 4.8% |
| MINW*4<Wage$\leq$MINW*5 | 10.5% | 2.9% | 11.0% | 2.6% |
| Wage>MINW*5 | 6.9% | 1.6% | 8.1% | 1.5% |

EoY: End-of-year wage change; Comp.: composite wage change.
Sources: SLID, authors' calculations.